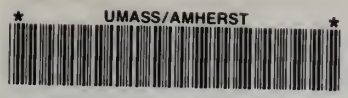
MASS. UM100.2: S131



312066 0278 1337 7

SILRESIM AREA HEALTH STUDY

REPORT OF FINDINGS

November 22, 1983

Report Prepared by

David Ozonoff, MD, MPH[1]

Mary Ellen Colten, PhD[2]

Adrienne Cupples, PhD[1]

Timothy Heeren, BA[1]

Arthur Schatzkin, MD, DrPH[1]

Thomas Mangione, PhD[2]

Miriam Dresner, MPH[1]

Theodore Colton, ScD[1]

GOVERNMENT DOCUMENTS
COLLECTION
DEC 19 1984
University of Massachusetts
Depository Copy

[1] Boston University School of Public Health - Prime Contractor

[2] Center for Survey Research, University of Massachusetts-Boston - Sub Contractor

Acknowledgements

The authors would like to extend our sincere appreciation to the people of our two study areas who gave us so willingly the benefit of their experience and took the time to respond to our lengthy interviews.

We would like to recognize the large number of other people who contributed to this report. The interviewers and staff of the Center for Survey Research carried out the field survey and coded, keypunched and cleaned the data tape. We would especially like to acknowledge the splendid work of Kim Streitburger and Getachew Demeke who drew the sample, supervised the coding and cleaned the data tape, and Dorothy Cerankowski who supervised the interviewers.

Elaine Trudeau, Director of the Registry of Vital Records and Statistics and Irene Klangos, Director of Public Information/Data Management, both of the Division of of Health Statistics and Research in the Massachusetts Department of Public Health were extremely helpful in providing the information which enabled us to look at mortality in our two study areas, and Rick Bird, Andy Epstein and Lee Tripp laboriously examined the 13,000 plus death certificates. Torie Ozonoff contributed at many points along the way through general comments and specific suggestions on methodology and analysis. Heather Hill typed the many versions of the final report, with its complex tables, with her usual professionalism.

The staffs of the two contracting agencies, the Department of Environmental Quality Engineering and the Department of Public Health have contributed in many ways to this effort and we gratefully acknowledge their support. We would also like to thank the members of the Lowell chapter of Massachusetts Fair Share for their energy and persistence in raising the issue.

Executive Summary

From 1971 to 1977 a large volume of chemical wastes was handled, often improperly, on a five acre site less than 400 feet from a densely populated residential neighborhood in the Ayer City district of South Central Lowell. Many of the chemicals posed a potential threat to health and the environment. In 1978 the Department of Environmental Quality Engineering (DEQE) secured the site and by September 1981 had all surface containers and their contents removed. The soil on the site remained contaminated, however, and residents continued to have concerns about past damages to their health and the possibility of further health risk from the chemical residues left behind.

On January 5, 1983 the Boston University School of Public Health contracted with the DEQE's Division of Water Pollution Control to study health effects in the vicinity of the site. Technical oversight was provided by the State's Department of Public Health, Division of Environmental Health Assessment.

From February 22, 1983 to April 20, 1983, the School and its subcontractor, the Center for Survey Research of the University of Massachusetts-Boston, surveyed a random sample of residents in the vicinity of the site ("the target area") and a second sample of Lowell residents at a distance from the site ("the control area"). By means of telephone and in-person interviews, residents in these two areas answered questions about their health and the health of their children, and provided additional background information on health-related characteristics such as smoking habits and occupational exposures. More than 80% of the 2463 eligible adults participated in the survey, providing information on just under 2000 adults and 1100 children in the two areas. In addition, we investigated differences in the patterns of causes of death between the target and control neighborhoods by examining death certificates of all Lowell residents who died between January 1, 1970 and December 31, 1981.

The health effects of most concern to neighborhood residents were adverse reproductive outcomes, especially stillbirths, miscarriages and birth defects, and serious diseases of adult life and childhood, particularly cancer and leukemias. The results showed that residents of the target neighborhood did not experience an unusual degree of reproductive problems or of cancer compared with residents of the control area. In addition, the mortality patterns did not differ in the two areas.

Since cancer involves a considerable lag time subsequent to exposure to carcinogenic agents (15-30 years) it would have been difficult to attribute any increase in cancer occurrence to the site, in any event. The lack of any demonstrable reproductive hazard is reassuring, as is the normal appearing mortality pattern. It should be emphasized that the sample sizes involved may not have been sufficiently large to detect small but real differences. The study team concludes that on currently available evidence there is no marked increase in reproductive hazard, cancer risk or mortality in the target area.

In addition to these major health effects, less serious but still troublesome health conditions were investigated. Adult residents of the target area were compared to those in the control area for self-reported symptoms occurring within the six months prior to the interview, and for disease conditions about which they were told by their doctor or other health care provider. Target area residents complained of respiratory symptoms more frequently than control area residents, including wheezing and tightness in the chest, cough lasting for more

than two weeks, persistent colds, and shortness of breath. Also more frequent in the Silresim area were constitutional complaints such as unusual fatigue and tiredness, bowel dysfunction and headaches. In addition, Silresim residents were told by their doctors that they had heart problems more often than the control population. The frequency of respiratory symptoms, headaches and fatigue was most elevated close to the site itself. This was true both for those residents who believed the air was unhealthy, and those who did not, indicating that bias was not the source of this effect. The influence of smoking, occupational exposures, age and a number of socio-economic factors was taken into account in these analyses.

Similarly, acute and chronic diseases of childhood were investigated through information supplied by the most knowledgeable adult of the household, usually the mother. Questions concerned childhood illnesses and conditions and were not the same as adult questions. There were significant differences between target and control areas in the frequency of in sore throats for both pre-school and school age children. For chronic conditions, a history of frequent bronchitis was more common in the Silresim area compared to control children.

Sources for the differences in the frequency of symptoms other than the Silresim site were considered. Differences in age, occupation, smoking habits and socio-economic status were taken into account in the analyses, and hence do not explain the observed differences in symptom prevalence. The tendency for target-area residents to have better recall of their symptoms than their control-area counterparts was considered in some detail. As a result of separate analyses that looked at those residents least likely to exhibit such recall bias, it was concluded that such an effect could not completely account for the differences, although some influence was evident.

On the other hand, a number of factors tended to reduce any real differences that might have existed as a result of the presence of the site. Only current residents were part of the study. Information from those who had moved away because of illness was lost. Furthermore, in most comparisons the entire target area was compared to the control, even though it is clear that there was differential exposure in the target area, with the more exposed "diluted" by the less exposed. Finally, the assumption that the control area was itself free of environmental hazard may not be true. If false, it would tend to reduce the differences found. For all of these factors, then, the tendency would be to mask any real effects.

The question thus remains whether exposure to chemicals from the waste site is the cause for this apparent increase in health problems among adults and children. While chemical emissions from the site are detectable, they are at an extremely low level. Exposures by routes other than air are even less likely. The existing literature on the chemicals present at the site suggests that symptoms would not occur until exposures were much higher than present at the site during the survey period. Thus, there seems to be only three possibilities to explain the results of the study:

(1) There may be an as yet undiscovered bias that distorts the data in such a fashion as to make an association with the site appear where none exists. Care has been taken in this study to eliminate or evaluate such effects, but it is impossible to be completely sure that none are operating.

(2) The air monitoring information may be unrepresentative. This seems unlikely. Monitoring was performed at the site and its environs on several occasions, using different methods. The results were all consistent.

(3) Health effects may appear at dramatically lower levels in a community population than in one occupationally exposed. Virtually all our current knowledge of health effects is derived from occupational studies. These might be inappropriate for use in a community setting, especially when complex mixtures rather than single agents are involved.

In summary, investigation of the Silresim area neighborhood did not reveal any unusual patterns of mortality, increased cancer risk, or birth defects or other reproductive hazards. Much less severe but still troublesome health effects did seem to be more common in the vicinity of the site. However, the observed effects seem to be out of proportion to the apparent exposure. Thus it is not possible at this time to state conclusively that the site is the cause of the adverse health effects.

TABLE OF CONTENTS

	Page
Executive Summary	
Introduction	1
Section 1: Site Conditions	2
Section 2: Study design	6
Hypotheses	
Study variables: Exposure	
Study variables: Health Outcomes	
Other study variables	
Statistical considerations	
Power Considerations	
Mortality Study	
Section 3: Data Collection Procedures	12
Sample	
Response Rate	
Letter about study	
Special procedures for phone and field	
Quality control	
Portuguese interview	
Appendix 3	
Section 4: Description of the Study Populations	17
Section 5: Findings: Adults	24
Univariate analyses	
Control of confounding and assessment of distance, direction and years-at-current address	
Section 6: Findings: Child health	34
Univariate analyses	
Control of confounding	

TABLE OF CONTENTS

	Page
Section 7: Findings: Birth Outcomes	42
Section 8: Discussion of Symptom Prevalence Survey	46
Section 9: Findings: Mortality Study	54
Section 10: Conclusions	58
Appendix 1: Health effects of some volatile chemicals present at the Silresim Site	
Appendix 2: Questionnaire	
Appendix 3: Letters and Factsheets	

Executive Summary

From 1971 to 1977 a large volume of chemical wastes was handled, often improperly, on a five acre site less than 400 feet from a densely populated residential neighborhood in the Ayer City district of South Central Lowell. Many of the chemicals posed a potential threat to health and the environment. In 1978 the Department of Environmental Quality Engineering (DEQE) secured the site and by September 1981 had all surface containers and their contents removed. The soil on the site remained contaminated, however, and residents continued to have concerns about past damages to their health and the possibility of further health risk from the chemical residues left behind.

On January 5, 1983 the Boston University School of Public Health contracted with the DEQE's Division of Water Pollution Control to study health effects in the vicinity of the site. Technical oversight was provided by the State's Department of Public Health, Division of Environmental Health Assessment.

From February 22, 1983 to April 20, 1983, the School and its subcontractor, the Center for Survey Research of the University of Massachusetts-Boston, surveyed a random sample of residents in the vicinity of the site ("the target area") and a second sample of Lowell residents at a distance from the site ("the control area"). By means of telephone and in-person interviews, residents in these two areas answered questions about their health and the health of their children, and provided additional background information on health-related characteristics such as smoking habits and occupational exposures. More than 80% of the 2463 eligible adults participated in the survey, providing information on just under 2000 adults and 1100 children in the two areas. In addition, we investigated differences in the patterns of causes of death between the target and control neighborhoods by examining death certificates of all Lowell residents who died between January 1, 1970 and December 31, 1981.

The health effects of most concern to neighborhood residents were adverse reproductive outcomes, especially stillbirths, miscarriages and birth defects, and serious diseases of adult life and childhood, particularly cancer and leukemias. The results showed that residents of the target neighborhood did not experience an unusual degree of reproductive problems or of cancer compared with residents of the control area. In addition, the mortality patterns did not differ in the two areas.

Since cancer involves a considerable lag time subsequent to exposure to carcinogenic agents (15-30 years) it would have been difficult to attribute any increase in cancer occurrence to the site, in any event. The lack of any demonstrable reproductive hazard is reassuring, as is the normal appearing mortality pattern. It should be emphasized that the sample sizes involved may not have been sufficiently large to detect small but real differences. The study team concludes that on currently available evidence there is no marked increase in reproductive hazard, cancer risk or mortality in the target area.

In addition to these major health effects, less serious but still troublesome health conditions were investigated. Adult residents of the target area were compared to those in the control area for self-reported symptoms occurring within the six months prior to the interview, and for disease conditions about which they were told by their doctor or other health care provider. Target area residents complained of respiratory symptoms more frequently than control area residents, including wheezing and tightness in the chest, cough lasting for more

(3) Health effects may appear at dramatically lower levels in a community population than in one occupationally exposed. Virtually all our current knowledge of health effects is derived from occupational studies. These might be inappropriate for use in a community setting, especially when complex mixtures rather than single agents are involved.

In summary, investigation of the Silresim area neighborhood did not reveal any unusual patterns of mortality, increased cancer risk, or birth defects or other reproductive hazards. Much less severe but still troublesome health effects did seem to be more common in the vicinity of the site. However, the observed effects seem to be out of proportion to the apparent exposure. Thus it is not possible at this time to state conclusively that the site is the cause of the adverse health effects.

Introduction

In 1974 unpleasant chemical odors began to be noticed in the vicinity of the Silresim Chemical Corporation at 86 Tanner Street in Lowell, Massachusetts. This site is located at the southern perimeter of an industrial area between the central business district and the residential areas in the southern section of the city. The operators of the Silresim Corporation, took in a wide range of industrial wastes for the purpose of reclaiming them by means of a treatment process to produce recycled chemicals of commercial value. Serious financial problems led to significant breaches of good waste management practices. One result was the strong smell in the neighborhood of the facility. Residents living near the site reported a variety of health problems that they associated with periods of increased odors from the plant site. A rough tally of pregnancy histories in the area by a community group suggested an unusual degree of reproductive failure.

State agencies first became involved because of citizen complaints. An investigation of operating conditions by the Department of Environmental Quality Engineering (DEQE) in 1976 resulted in a concerted attempt by its Division of Water Pollution Control to revoke Silresim's permit, but this was stymied by the Court. When operations finally ceased at the end of 1977 because of the Corporation's bankruptcy, the full extent of the environmental problem was revealed to public health and environmental protection authorities. The discovery of extensive contamination of the site and its vicinity by a wide range of toxic chemicals added a new dimension to community complaints and suspicions. The final removal of material remaining in the abandoned containers in September, 1981, was an important remedial step, but it addressed neither the damage to health that could have occurred in the period prior to removal, nor the potential for further health damage from the residues of spilled toxic materials that remained in the soil matrix and within the groundwater beneath the site and its environs.

On January 5, 1983 the Boston University School of Public Health contracted with the Department of Environmental Quality Engineering's Division of Water Pollution Control to study health effects in the vicinity of the site. From February 22, 1983 to April 20, 1983 the School and its sub-contractor, the Center for Survey Research of the University of Massachusetts-Boston surveyed residents in the vicinity of the site ("the target area") and a second group of Lowell residents at some distance from the site ("the control area"). In addition, mortality differences between target and control residents were investigated by examining the death certificates of all Lowell residents who died between January 1, 1970 and December 31, 1981. This report presents our findings as of November 1983.

SECTION 1

SITE CONDITIONS

From 1971 to 1977 a large volume of chemical wastes was handled, often improperly, on a 5 acre site less than 400 feet from a residential neighborhood in the Ayer City district of South Central Lowell (Figure 1). Many of the chemicals posed a threat to health and the environment and were on EPA's list of 129 Priority Pollutants.[1] The site itself is on a portion of a glacial lake deposit consisting of an approximately one hundred foot thick layer of sand and silt strata overlying bedrock.[2] The water table is generally close to the surface, varying in depth from 2 feet to 5 feet. Groundwater appears to flow in a northward and downward direction.[3] The Northeast region of the United States is a high runoff area, and Lowell receives nearly 95 cm. precipitation yearly. The Silresim site has no vegetation so there is little evapotranspiration, and the site's flat contour and pervious soil promote infiltration.[4] The gross contamination of the site resulted in large part from this combination of high annual rainfall, shallow groundwater, and porous surface soil.[5]

New spills of chemical contaminants probably stopped after the site was secured at the time of its abandonment in 1978, although air emissions from the on-site chemicals occurred to some extent until their final removal in September of 1981. In October 1981 DEQE contracted with the engineering consulting firm of Perkins Jordan, Inc. to determine the nature and extent of the remaining contamination of soils and groundwater, and to suggest remedial actions. The Perkins-Jordan Report examined the hydrogeological problem and generally did not concern itself with air emissions, although some data were available that could be applied to estimate the extent of airborne exposure.

The mechanisms whereby chemicals can move off-site* and expose neighborhood residents are limited in number: [1] infiltration to the groundwater and then migration offsite; [2] surface runoff followed by volatilization or downward percolation to groundwater; and [3] volatilization from the soil matrix and dispersion through the atmosphere.

An evaluation of the site conditions suggested that if any exposures were occurring, the most plausible route would be through the air. This conclusion was based on the following considerations:

[1] The bulk of site-derived chemicals in groundwater are kept near the surface by a relatively impermeable layer of silt at a depth of about 15 feet.[7] Plume patterns suggest that migration is controlled by the processes of hydrodynamic dispersion and groundwater flow.[8] Contaminants have already moved a small distance north of the site via the groundwater.[6] The measured horizontal rate of 8 to 16 feet per year for shallow flow makes it unlikely that there has been, or will be for many decades, significant direct human exposure by chemicals in the contaminant plume. However, some of the volatile components near the water table surface can escape to the voids in the zone of unsaturation and thence to the atmosphere.

[2] Surface drainage from the Silresim site is to the west and north toward River Meadow Brook 350 feet away.[9] Dead vegetation and soil odor and discoloration immediately outside the site perimeter, especially to the north, are evidence that some contaminants have migrated a short distance via surface runoff. Not all contaminants move with equal ease, with dissolved species such as dichloromethane (methylene chloride) being more mobile than immiscible

*Some on-site exposures may have occurred by children playing on the property before it was secured.

compounds such as the PCBs.[10] Compounds can also volatilize and be dispersed through the air after being deposited offsite by surface runoff.

[3] Volatile compounds can be released to the atmosphere from above-ground containers, the soil matrix, or the surface of the water table. They can then be dispersed offsite by prevailing winds which are generally from the west, shifting from southwest in spring and summer to the northwest in fall and winter, although at a given time they can come from any quarter.

Odors emanating from the site were in fact caused by airborne contaminants that exceeded the sensory threshold, so it is clear that airborne exposures did occur to some extent. On the other hand, the fact that groundwater neither serves as a source of drinking water for the neighborhood (which is served by the municipal system) nor is it contaminated by a plume of sufficient areal extent to present even the remote possibility of direct exposure via subsurface seepage into basements, means that groundwater is not a likely exposure route. Likewise, while there is evidence of runoff resulting in some contamination adjacent to the site, this is not in areas where many neighborhood resident might be exposed. These considerations led to the conclusion that whatever exposures were occuring were most likely through the air.

To verify this, air monitoring studies around the Silresim site were conducted on three occasions. In mid-August 1982 the DEQE conducted ambient air measurements at five sites in Lowell, one at a station at the site itself, three stations in the immediate neighborhood of the site, and one background station used for comparison purposes. Benzene, toluene, trichloroethylene and tetrachloroethylene were detected, but nowhere at levels significantly different than background. Meteorological data collected confirmed that the predominant wind direction was from the west and southwest. A second study was performed in November of 1982 by a private contractor to DEQE using a different air monitoring instrument. This study showed what appeared to be aliphatic amines at levels of hundreds of parts per billion (ppb) and phthalic anhydride/total phthalate compounds at concentrations of up to 100 ppb. To resolve the doubts caused by this second study, a third air monitoring study was conducted in March, 1983, simultaneously with the health interview survey. This was a comprehensive study using four different sampling techniques and analytical methods. The results of this investigation tended to confirm the first DEQE study, but also uncovered new sources of air contamination from two industrial facilities located to the northeast of the Silresim site. These factories were releasing significant quantities of the solvents dimethyl formamide (DMF), toluene and xylene into the surrounding areas. At times the contaminant plume from these facilities could be detected drifting down over the Silresim site and its environs, and the location of the facilities made it clear that a portion of the control area population was being exposed to very high levels. The presence of DMF in the ambient air also explained the anomalous results of the November study. In hindsight it appears that what was actually measured at that time was the unsuspected DMF, not the amines and phthalic anhydrides.

In summary, evaluation of the conditions of exposure at the Silresim site suggested that if exposures to the population were occurring the principal route would be through the air. Air monitoring studies were performed but not until well after the site had been secured and surface containers removed. These studies failed to detect air contaminants from the site at levels that could explain the suspected health effects. However, a new source of airborne contamination was discovered in the control area northwest of the site.

References

1. Perkins, Jordan, Inc., Hydrogeological Survey, Feb. 26, 1982. Appendices, B3 and B6.

2. Ibid, pp. 9-10.

3. Ibid, pp. 11-12.

4. Ibid, p. 3.

5. Ibid, p. 20.

6. Ibid, Figure 12.

7. Ibid, p. 19.

8. Ibid, pp. 15-16.

9. Ibid, p. 3.

10. Ibid, p. 20.

11. Ibid, p. 21.

12. Ibid, p. 15.

SECTION 2

STUDY DESIGN

The overall objective was to assess the impact on the health of area residents exposed to airborne contaminants from the Silresim site. A cross-sectional survey was used to examine the relationship between residence near the site and self-reporting of selected symptoms. Both acute symptoms, reflecting current exposures, and chronic conditions that might have resulted from past exposures were included.

Hypothesis To be Tested

Exposure to airborne chemicals from the Silresim site is associated with increased prevalence of self-reported adverse health outcomes and/or altered patterns in cause of death for deceased residents.

Study Variables - Exposure

The most obvious index of potential exposure was residence near the Silresim site itself. The target area was defined as households within a 1/4-mile distance of the site. Because it was not at all certain how far the influence of the site extended, the linear distance of each residence to the center of the site was measured on a map and became a second independent variable. Since airborne contaminant exposures could also be affected by wind direction, the various directions around the site were divided into four quadrants. Finally, health effects might be affected by the length of exposure, so years-at-the-current-address was added as an exposure variable.

The main criterion for the control area was that it not be influenced by contamination from the site. Crude worst-case estimates of airborne pollutant behavior indicated that a distance of 1/2 mile would be ample separation from the site and yet provide a residential area sufficiently similar for comparison purposes. Rather than designate a compact contiguous area as a control, a large ring-shaped area completely surrounding the site in a band from 1/2 to 3/4 of a mile was chosen (see Figure 2). This was done to avoid the possibility that some more discrete comparison area might by chance have an unrecognized environmental problem of its own. Our choice of control area tended to "spread the risk" of this eventuality. As it turned out, the discovery of the DMF emitting factories in quadrant 1 of our control area was just such an event. The design allowed us to perform our analyses both with and without this portion of the control area.

Since all portions of the control were of approximately equal distance from the site, and as far as exposures were concerned were at essentially "infinite" distance, they were all coded alike in a way which reflected this. To do this, the reciprocal of the distance was used as a variable, leading to positive numbers for target households and zero values for control households. This also meant that as distance from the site increased, the distance variable decreased.

Study Variables - Health Outcomes

The airborne exposure route implied that the volatile chemicals would pose the most likely threats to health. Volatile species at the site included benzene and its derivatives and several straight-chain chlorinated hydrocarbons. The extensive literature on the known contaminants at the site helped guide the choice of health outcomes. Appendix 1 gives a brief summary of this literature.

Neighborhood concerns were also important in selecting health outcome variables. Frequent complaints of skin irritation and rashes, eye and nose

irritation, headaches, upper respiratory infections, asthma, bronchitis, high prevalence of cancer and reproductive failure were obtained in an informal health survey of the neighborhood by a community group, Massachusetts Fair Share. While some of these complaints, such as eye and nose irritation, are plausible results of exposure to some of the chemicals in question others are less obviously related.

Questions about adverse health outcomes in this study can be roughly divided into those that are of relatively short duration, and those that are more chronic or of such magnitude as to be easily remembered. In the first category are a wide variety of acute symptoms for which respondents were asked to confine themselves to occurrences only within the previous six months. In some instances, such as reasons for doctor visits, an interval of two months was specified. This was done to minimize possible recall bias. In the second category are questions that asked if a doctor had ever diagnosed a major chronic disease such as heart disease, or if there was a history of miscarriage, birth defect or infertility.

A summary of the health outcome variables is presented in Table 2-1. The full text and explanatory phrases can be found in the questionnaire, given in Appendix 2.

The Question of Verifying Health Outcomes

The self-reported health problems were generally not subject to verification. Most involved complaints such as headaches, dizziness or fatigue, for which objective findings do not exist. Respondents were asked whether they sought medical evidence for these complaints, but the decision to see a doctor is influenced by many factors other than the actual existence of the complaint. In any event, such care-seeking behavior results in a documentation of a self-reported illness with no more objective foundation than that obtained by survey methods.

The answers to questions concerning whether a doctor or other health care provider had ever diagnosed certain diseases is, in principle, verifiable, but the process is time consuming and expensive. In the present instance these outcomes were not strikingly different in the target compared to the control groups, so detailed verification was not carried out.* For reproductive outcomes and mortality vital records exist and can be used. In the case of reproductive outcomes there were no plausible differences so no verification was attempted. The use of death records to study mortality, is described below.

Finally it should be noted that the perception or self-report of ill health is an objective fact on its own of some significance and requires no verification. This does not deny that differences in self-reporting might be due to bias of some kind. This is addressed in section 8. But insofar as some kind of bias is not at work to produce an artifactual difference, there is no question that perception of symptoms, whether or not there is a documentable physiological basis for it, has an impact on the quality of life, the efficiency and the productivity of a community.

---

*The exceptions were differences in medical histories of anemia and other blood problems in adults (see section 5), but these showed evidence of being the result of bias (see section 8).

Other Study Variables

A number of background characteristics of the target and control populations also influence the prevalence of symptoms independently of any effect of the site. Special efforts were made to collect information on these potential "confounding" variables. They are also given in Table 2-1 and Appendix 2.

In addition, several questions were asked to elicit respondents' awareness and attitude about their neighborhood environment, specifically, whether they noticed problems with their drinking water or the air they breathed, and their beliefs as to whether the water and/or air made them ill.

Statistical Methods

Target and control areas were compared on both crude and adjusted prevalence rates. Crude rates were compared through the chi-square test of independence. This procedure assumes that each respondent constitutes an independent response. Since data were collected on a household basis, and some correlation between responses from the same household may exist, this procedure may exaggerate significance levels. To account for this sampling procedure, we also performed analyses (weighted t-tests) treating the household as the unit of analysis.

Multiple logistic regression was then used to compare prevalence rates between areas adjusting for the background factors age, sex, education, marital status, smoking status, type of house, and work exposure. Age is represented in these analyses as a measurement variable, while all other factors are represented as dichotomous variables. A dichotomous variable was also used to distinguish between the target and control areas. The size of the difference between areas, accounting for the background factors, is measured through the odds ratio associated with the target/ control variable. An odds ratio is a measure of strength of an association. In this case an odds ratio of 1.4 would mean that the odds of having a condition are 40% higher in the target area compared to the control. Additional logistic regressions incorporating various interaction terms were performed to investigate possible associations between outcomes and distance or direction from site. Multiple linear regressions were also used to analyze birthweight.

Power Considerations

This study is comparing two groups of approximately 1,000 respondents each. The following table presents differences between areas that are detectable with 90 percent power through a two-sided 0.05 level chi square test of independence:

Prevalence rate in control area	Prevalence rate in target area	Prevalence rate ratio
.10	.15	1.50
.20	.26	1.30
.30	.37	1.23

Mortality Study

All death certificates for Lowell residents who died between January 1, 1970 and December 31, 1981 were examined to determine if the decedents lived in either the target or control areas. The deaths in these two areas were compared for differences in the proportions of various causes of death to see if residence in the target area had affected the mortality experience of the neighborhood.

TABLE 2-1

Adult Health Variables

Comparative and Overall Health

In the past 6 months:
- wheezing/tightness in the chest
- chest pain
- nausea or vomiting
- dizziness
- shortness of breath
- numbness fingers and toes
- unconsciousness or blackouts
- swollen glands
- skin irritation
- irritation eyes/nose
- cough, 2 weeks or more
- irregular heart beat
- loss 10 lbs without dieting
- fatigue
- nervousness
- acne
- unusual color urine
- bruising/bleeding
- persistent colds
- headaches
- other

Has a doctor ever told you you had:
- Anemia
- other blood problems
- kidney problems
- respiratory problems
- neurologic problems
- skin disease/allergy
- diabetes
- hypertension
- high cholesterol
- liver disease
- digestive disease
- cancer (what kind?)
- heart trouble (what kind?)

Have you seen a doctor in past 2 months (for what)?

Hospitalized since June?

Miscellaneous

Pet deaths and illnesses

Awareness and attitude towards neighborhood environment: air and water

Reproductive History (Women Only)

Miscarriage
Stillbirths
Infant and childhood mortality
Prematurity
Birth weight

Child Health Variables

History of:
- epilepsy
- asthma
- bronchitis >3x/yr.
- not developing normally
- dyslexia
- hyperactivity
- mental retardation
- hearing loss speech impediment
- "slow"
- other
- earaches
- sore throats
- frequent colds

Uncommon childhood illness of:
- bone
- lungs
- skin
- glands
- other

School absences

Condition that limits normal activity

Confounding Variables

Age
Sex
Race/Ethnicity
Marital Status
Income
Housing
Education
Smoking History
Alcohol Use
Occupational/Avocational Exposures

SECTION 3

DATA COLLECTION PROCEDURES

From February 22 to April 20, 1983, the Center for Survey Research conducted 1997 interviews in Lowell. 808 interviews were done on the telephone and 1189 in the respondent's households. 1049 interviews were with individuals in the "target" area around the Silresim waste disposal site and an additional 948 were with individuals in the designated control area in Lowell. All procedures and materials for the target and control areas were identical. Interviewers conducted interviews in both areas and during the same time span. Attempts were made to interview every person over 18 within every household in the target area and within the randomly selected households in the control area. Each individual interviewed was asked about his or her own health, residential and occupational history, opinions about contaminants in the air and water, and demographics. Within each household, the first individual interviewed was queried about the dwelling unit, pets, and household practices concerning water use. One adult in the household provided the data on all the children under 18 residing in the household. If the first adult interviewed felt unable to provide the information, it was obtained from an adult who felt qualified to do so.

In this section we describe the sample procedures, sample sites, response rate, and quality control procedures.

Sample

Respondents were drawn from two areas in Lowell. The "target" area included all households in all blocks which fell completely within 1/4 mile of the Silresim waste disposal site. The "control" area was a ring with the innermost boundary at least 1/2 mile from the site and the outermost boundary at least 3/4 mile from the site. Blocks which spanned the boundary of the area were not included in the sample.

The sample frame was drawn in the field. Field personnel were given lists of addresses on each block in the area which were taken from the city directory. The lister then went to his/her assigned area and made additions or deletions according to what actually existed at the time. Later, "found households" were also added to the sample frame. "Found households" are households uncovered by field personnel by systematic questioning which were not included in the original listing because they do not appear as separate dwellings. An "in-law apartment" without a separate address, mailbox, (or phone) is an example of a household which might fall into this category.

There were 605 households in the 34 block target area, all of which were included in the sample. In the 94 block control area, there were approximately 2800 households which were randomly sampled at a rate of 1 in 4 to yield 649 households.

Response Rates

While the sampling unit for this study was households, the goal in the study was to interview every adult in every household in the sample. Thus, the individual response rate is based on the number of eligible adults in the designated households. For some households, it was either not possible to contact anyone in the household or an interviewed member refused to provide information about the number of adults in the household. Thus, it was necessary to estimate the number of adults in each of those households (all of whom were not interviewed and are thus counted as non-response). For the estimate of number of adults in the household, the mean number of adults in households in

that area was used. Thus, for every household in the target area for which there was no contact or a refusal without obtaining information about the number of adults, the number of non-respondents was estimated to be 2.18, which was the average number of adults in households in the target area for which the number of adults was ascertained. Similarly, in the control area, the estimated number of adults in households where the number of adults was known was 1.97.

Table 3-1 shows the calculated response rate for individuals based on these estimates. Line 1 is the total sample from the lists plus "found" households. Line 2 is the households from the listing which were established by interviewers to be non-dwelling units or vacant. Line 3, then, is the resulting sample frame. Line 4 is the households for which we were unable to ascertain the number of adults. Line 5 is all of the households for which we were able to obtain information on the number of adults, even if it was impossible to obtain an interview with any or all of them. Line 6 is the total number of known adults in those households and line 7 is the average number of adults in those households. This estimate times the households with unknown adults (line 9) yields the number of estimated adults in the sample. Adding this to the known adults in the sample (lines 6 and 9) gives the estimate of the total eligible adults in line 10. The total eligibles divided into the total interviews yields the response rates shown in line 12. The response rate for the control area was 79.1% and 83.0% in the target area. Explicit refusals were less than 4% in both areas. These refusals were evenly divided between the two study areas. There is no evidence that there was selection bias introduced by this low refusal rate.

Letter About the Study

A letter was sent to each sampled household explaining the study and telling them to expect to hear from an interviewer. Also included was a fact sheet about the study. For households later identified as Portuguese, a Portuguese letter was delivered by a Portuguese interviewer. When a refusal was received from a household, a follow-up letter was sent before another attempt to obtain an interview was made (see Appendix 3).

Special Procedures for Phone and Field

Because telephone interviewing is considerably less costly, attempts were made to do as much interviewing over the phone as possible. Since the sample unit was an address, procedures were followed to obtain phone numbers for the household when possible. This resulted in a combination of phone and in-person interviews. This is acceptable survey research practice if the same interview is used for both, as it was in this study (cf. Mangione et al., 1982; Thornberry, 1976).

Attempts were made to obtain a phone number for each of the addresses in the sample through town lists and directory assistance. "Cover sheets" were then prepared for each household in the sample frame. These sheets included contact information and were kept separate from the interview. All cover sheets for which we could obtain a phone number were initially assigned to phone interviewers. Those for which we were unable to obtain a phone number were assigned to the field.

For each coversheet assigned to the field, the interviewer went to the household. If contact was made, the interviewer was instructed to take interviews with all adults present at the time and to obtain a phone number in order

to contact by phone any adults who were not interviewed at the time of the visit. Interviewers made a maximum of six visits to a household, well spread over time, before designating the household as a "no-contact".

For coversheets which were initially assigned to the phone, five well spread calls were made, varying the time of day and including both weekdays and weekends. If no contact was made after five calls, the phone number was verified with the telephone company. If the phone number was incorrect or not working, the coversheet was assigned to the field and procedures followed as if it originated in the field. If the phone number was correct, five additional well-spaced phone calls were made before assigning the coversheet to the field. For all phone interviews the interviewer first verified that he or she had, in fact, reached a phone number at the correct address. To reach subsequent adults in the household, up to ten additional calls were made.

Coversheets moved from the phone to the field under two other conditions. One was for Portuguese respondents who needed to be interviewed in Portuguese. The other was for refusal conversion in the control area. Refusal conversion is the attempt to convert an initial refusal with an interview. For every refusal, at least one attempt is made, by a different interviewer from the one who received the refusal, to convince the respondent to participate. Since refusal rates in the control area were initially higher and in-person response rates tend to be slightly better, phone refusals in that area were transferred to the field for conversion attempts.

A total of 1189 interviews were conducted in-person and 808 conducted by telephone.

Quality Control

All Center for Survey Research interviewers receive a minimum of 3-5 days training in general interview techniques. Additionally all interviewers, including experienced ones, received one day training on this study.

Field interviewers worked at least one six hour shift on the telephone so that their interviewing could be monitored. Their completed interviews were reviewed as they were returned to the office. Each field interviewer had at least weekly contact with a supervisor.

During phone shifts, a supervisor was always present in the room. At least one interview was monitored by the supervisor for each shift the interviewer worked and two additional completed interviews per interviewer were carefully reviewed each shift. Specific areas of interviewing technique were rated and immediate feedback was given to each interviewer by the supervisor.

Approximately 10% of the sample was recontacted to ensure that they were, in fact, interviewed.

Portuguese Interviews

Lowell has a substantial number of Portuguese residents. In cases where the respondent was not fluent in English, the interview and the respondent letter were translated into Portuguese, and interviews were conducted in Portuguese by three Portuguese interviewers.

TABLE 3-1

Lowell Field Results

	Control	Target	Total
1. Household Sample	649	605	1254
2. Non samples (not dwelling or vacant)	42	24	66
3. Total Eligible Households	608	580	1188
4. Households - # Adults not ascertained	32	23	55
Refusal	(24)	(17)	(41)
Language	(2)	(1)	(3)
Limit	(3)	(4)	(7)
No contact-limit	(1)	(1)	(2)
Other	(2)	(0)	(2)
5. Households - # Adults known	576	557	1133
6. # Known Adults in Household	1135	1215	2350
7. Average # Adults in Household	1.97	2.18	2.07
8. # Known Adults in Household	1135	1215	2350
9. Average x Non-interviewed Households	63	50	113
10. TOTAL ELIGIBLE SAMPLE	1198	1265	2463
11. INTERVIEWS TAKEN	948	1049	1997
12. RESPONSE RATE	79.1%	83.0%	81.0%

SECTION 4

DESCRIPTION OF THE STUDY POPULATIONS

In this section we summarize the sociodemographic and behavioral characteristics of the two study area populations (Tables 4-1, 4-2, and 4-3).

The total sample consisted of 1015 households, 502 in the control area and 513 in the target area. These households contained 1997 adults and 1106 children, 1049 adults and 594 children in the target area and 948 adults and 512 children in the control area. Sex and marital status distribution for adults were similar for target and control areas. The age and sex distributions for children were also comparable for the two areas.

There were, however, some statistically significant differences between the target and control areas. The target area had significantly more single-family houses, while the control area had more apartments (the percentages of 2- and 3-family houses were similar). The target area also had more multiple-adult houses. There were more long-term residents in the target area, with 40% residing at their current address since 1970 as compared to only 30% in the control, and the population was slightly older than the control area population. The percentage employed was the same in the target and control areas, but of those persons not working, more were retired in the target area while more were laid-off in the control area. Both areas are over 90% white; among the small percentages of nonwhites, the target had a higher proportion of blacks, while the control had more Hispanics. These findings suggest that the control area is one with more newcomers. The greater proportion of households with incomes under $15,000/year and adults with less than a high school diploma in the control area is consistent with this characterization.

The target area had significantly more current smokers than the control area (46.7% vs. 40.9%,) and there was a slight tendency to take alcoholic beverages less frequently, although the average number of daily drinks was the same in both areas. Although residents in the target area spontaneously mentioned the Silresim site or Mass Fair Share three times as often as those in the control area (27% vs. 10%,) there was no difference in the interviewer's perceptions of cooperativeness in the two groups. There was no difference in the frequency of occupational exposures between the two areas.

The overall picture, then, is of a target area that is somewhat older, more stable, marginally better educated and slightly more affluent and smokes more than the control area.

Although the differences between the two populations were not great, the fact that some of the factors on which the populations differed could be related to health outcomes suggested the need for statistical control of these factors in the analysis. Discussion of the multivariate analysis of these potential confounders can be found in Section 2.

TABLE 4-1

Characteristics of Target and Control Populations

	Target %	Control %	p*
Sex - Adults, 18 and over	(N = 1049)	(N = 948)	ns
Male	48.0	47.9	
Female	52.0	52.1	
Marital Status			ns
Married	58.7	54.6	
Widowed	6.9	7.2	
Separated	3.2	4.0	
Divorced	6.8	6.9	
Never Married	24.4	27.3	
Sex - Children, 0 - 17	(N = 594)	(N = 512)	ns
Male	49.2	50.4	
Female	50.8	49.6	
Age - Children			ns
0 - 2	13.6	17.4	
3 - 5	17.9	19.0	
5 - 10	27.1	26.0	
10 - 15	28.5	27.7	
15 - 17	13.0	10.0	
Housing	(N = 513)	(N = 502)	< .0001
Single family house	50.6	30.8	
2,3-family house	38.2	38.6	
Apt/flat	11.2	30.6	
Number adults in house			< .01
1	22.0	29.9	
2	52.4	48.2	
3	13.8	14.7	
4+	11.8	7.2	
Years at current address			< .001
Since 1970	40.8	30.5	

TABLE 4-1 (continued)
Characteristics of Target and Control Populations

	Target %	Control %	p
Age - Adults, 18 -			.01
18 - 19	6.6	4.2	
20 - 29	29.3	33.5	
30 - 39	19.0	20.5	
40 - 49	15.4	12.1	
50 - 59	11.6	14.4	
60 - 69	11.4	9.4	
70 - 79	5.1	4.4	
80 - 89	1.5	1.5	
Currently employed	62.9	60.2	ns
Why unemployed			.02
Unemployed	13.7	14.5	
Laid-off	7.0	13.4	
Retired	31.2	22.6	
Medical leave	3.6	3.0	
Disabled	10.1	11.3	
Homemaker	28.3	31.4	
Student	5.9	3.5	
NR	.3	.3	
Ethnicity			.0001
White	92.0	90.4	
Black	4.7	1.3	
Hispanic	2.3	6.4	
Native American	.6	.6	
Asian	.5	1.3	
Family Income***			.01
- 5,000	8.1	8.6	
5,000 - 10,000	17.9	20.5	
10,000 - 15,000	19.4	23.5	
15,000 - 20,000	14.8	11.8	
20,000 - 25,000	19.8	16.7	
25,000 - 30,000	7.2	9.2	
30,000 +	9.7	12.8	
Education			.003
- 8th grade	17.0	22.5	
1-3 years high school	27.4	25.1	
high school diploma	40.4	34.6	
1-3 years college	11.4	11.9	
college diploma	2.6	4.4	
graduate school	1.2	1.6	

*x^2-test, 2-tailed; ns means $p > .05$.
**Maximum n. Actual n varies slightly from item to item because of occasional missing data.
***N=1777. This item had significantly more missing data points than other items (220 missing values).

TABLE 4-2

Smoking, Drinking and Occupational Exposures

	Target % (N = 1049)**	Control % (N = 948)**	p*
Current Smokers	46.7	40.9	.0001
Quitting rate of ever smokers	70.3	71.1	ns
Drinking frequency/mo.			.02
non-drinkers	32.4	33.8	
1 - 5/mo	34.4	30.4	
6 - 10/mo	13.8	11.9	
11 - 20/mo	8.1	8.0	
21 +/mo	11.4	15.9	
Number of daily drinks			ns
0	32.3	33.7	
1 - 2	29.9	31.5	
3 - 6	31.8	27.3	
7+	6.0	7.5	
Occupational Exposures:*** Ever exposed to			ns
Smoke, fumes, vapors	41.1	40.6	
Don't know	.6	.4	
Pesticides or Herbicides	4.7	5.4	
Don't know	1.1	1.0	
Irritants to eyes or nose	22.8	24.0	
Don't know	.2	.1	
Asbestos	8.7	8.1	
Don't know	2.0	3.2	
Beryllium	1.2	.8	
Don't know	7.1	8.1	
Radiation	2.4	4.5	
Don't know	2.4	4.7	

TABLE 4-2 (continued)
Smoking, Drinking and Occupational Exposures

	Target % (N = 1049)**	Control % (N = 948)**	p*
Sandblasting, silica, rockcrushing, rockdrilling talc	11.3	9.8	
Don't know	.4	.7	
Solvents/degreasers such as perc or trichlor	20.1	16.7	
Don't know	1.4	2.1	
Lead	6.9	6.1	
Don't know	.6	.1	
Mercury, Cadmium, Arsenic	2.3	2.7	
Don't know	1.0	2.1	

*x^2-test, 2-tailed, ns > .05
**Maximum n. Actual n varies slightly from item to item because of missing data.
***Percentage not exposed to each agent has been omitted for clarity.

TABLE 4-3

Knowledge or attitudes of respondents that might affect reporting

	Target % (N = 1049)	Control % (N = 948)	p*
Mentioned Site or Mass Fair Share spontaneously	27.5	9.6	.0001
Cooperativeness as perceived by interviewer			ns
Cooperative	87.3	85.8	
Neutral	10.6	12.6	
Antagonistic	2.0	1.6	

*x^2, 2-tailed, ns > .05

SECTION 5

FINDINGS: ADULTS

Univariate Comparisons

Table 5-1 gives a comparison between target and control areas for self-reported symptoms in adults within the previous 6 months, and for disease conditions about which the respondent was told by his/her doctor or other health care provider. In this crude comparison, the target area residents complained of respiratory symptoms more often than the control residents, and there were also differences in reporting of heart problems, and constitutional complaints such as tiredness and bowel disorders. By contrast there were no significant differences in most other gastro-intestinal symptoms or problems, and none referable to the nervous system. Analyses accounting for within household correlation is presented in Table 5-2. Results are very similar to the chi square analysis.

Control of Confounding and Influence of Additional Independent Variables

Using the technique of multiple logistic regression we controlled for the following characteristics of the population: sex, age, marital status, education, type of house, exposure to hazards at work, and smoking habits. Income was used in many regression runs and produced results consistent with those runs where it was not included. Because many of the other variables are highly correlated with income and there were many missing values (see Table 4-1), it was eliminated in the regression so as to include the maximum number of households in the analysis. Employment status was also correlated with other variables and not used in the regression runs. Both areas were over 90% white, so that ethnicity varied too little to include it as a variable. The measure of drinking habits was rather crude and was not much different in the target and control areas, so it too was eliminated in the regressions. Table 5-3 shows that all variables which were previously significant at the $p = .05$ level remain so after taking potential confounding variables into account. The odds ratios calculated from the regression coefficients are quite close to the univariate odds ratios from the crude analysis. Table 5-4 compares the odds ratio for residence in the target area to those of significant background variables.

A second set of multiple logistic regressions was performed excluding the northeast quadrant of the control area which contained the solvent emitting factories. The coefficient for "irritation and burning of the eyes" became significant and positive, suggesting that the factories exerted a blocking effect when their influence was included in the control area (see last line, Table 5-3). "Anemia" and "other blood problems" fail to be significant when this quadrant is eliminated. Otherwise, the comparisons against the control area minus quadrant 1 do not alter which conditions have a significantly higher prevalence in the target compared to the control.

The same regression technique was used to assess the influence of the other independent variables: distance from the site, direction in relation to the site, and length of residence at current address (a surrogate for duration of exposure). These were added to the model that included the background variables and residence in target or control areas. Table 5-5 shows what happens when distance is added to the model. For the "wheezing" and "fatigue" outcomes, knowledge of proximity to the site gives additional information beyond that provided by knowing whether the residence was in the target area or not. Proximity to the site was a significant variable for four other variables which were not previously related to residence in the target area. This suggests that for these effects (chest pains, shortness of breath, easy bruising/bleeding, headaches) the target area included many people who were not influenced by the site, namely

those more distant. Table 5-5 shows a distance gradient in prevalence for areas close to the site compared to other areas. The odds ratios comparing risk of those close to the site with risk of controls in Table 5-5 also show that the effect of excluding the outer edges of the target area increased the odds ratios between the target area and control area. Whether this is a biological effect or somehow connected with awareness of the site is discussed further in Section 8.

Additional models were fit to investigate various interactions involving years-at-address, direction and distance. A significant interaction was observed between years-at-address and target-or-control residence for reporting of "unusual color to urine" and "irritation of eyes and nose". For both these outcomes, complaints increased with years-at-address in the target area, while complaints were unaffected by years-at-address in the control area.

Significant interaction was observed between quadrant 1 (downwind) and target-or-control for "irritation of eyes and nose". The results indicate that controls in quadrant 1 have higher rate of eye and nose irritation. This is consistent with the known presence of the two factories which emit mucuous membrane irritants into this quadrant of the control area. Significant interaction was also observed between quadrant 3 (upwind) and target-or-control for "neurologic problems" and "digestive problems." These analyses imply that controls in quadrant 3 reported higher rates of neurologic problems, while quadrant 3 targets reported lower rates of digestive problems than other respondents (see Table 5-6). Thus, neither of these interactions supports the hypothesis of a medical history of direction-associated neurologic or digestive problems in the target area.

An examination of the "other symptoms" category did not reveal any pattern of specific medical conditions that differed markedly between target and control areas. The same is true of heart disease. In both cases specific symptoms or heart conditions were too rare for analysis.

Thus, the findings in the adult health section of the study show significantly higher site-associated prevalences of the following cardio-respiratory complaints: wheezing/tightness in the chest, cough, persistent colds, chest pains, shortness of breath, and irregular heart beat; the constitutional complaints of bowel disorder, fatigue and tiredness, and headaches; easy bruising and/or bleeding; and a medical history of anemia and other blood problems. In addition, after target-control residence is controlled, six conditions show increased prevalences with closeness to the site. They are: wheezing/tightness in the chest, chest pain, shortness of breath, fatigue, easy bruising and headaches. Both "irritation of the eyes and nose" and complaint of "unusual color to the urine" were increased in the target area with increasing years of residence. "Irritation and burning of the eyes and nose" was of higher prevalence in the target area after the factory quadrant was eliminated from the comparison group. Analysis of the interaction variables also suggests that quadrant 1 of the control area has higher prevalence of eye irritation, than other quadrants.

Finally, the analysis demonstrates that the revealed differences are not the result of spuriousness produced by the usual confounding variables. There is still a possibility, however, that the data are biased in some other way to produce these results. This will be discussed in Section 8.

TABLE 5-1

Association with the target area, univariate comparisons

Outcome N = 1997**	Target % (N = 1049)	Control % (N = 948)	Prevalence Ratio, target/control	p*
Respiratory symptoms/problems				
Wheezing, tightness in chest	27.4	20.6	1.33	.0005
Cough, more than 2 weeks	25.6	18.8	1.36	.0004
Persistent colds	11.8	7.6	1.55	.002
Shortness of breath	24.5	21.5		ns
Lung problems, told by doctor	15.3	13.3		ns
Cardiovascular symptoms/ problems				
Irregular heart beat	14.9	11.4	1.31	.03
Chest pains	19.4	17.6		ns
Heart trouble, told by doctor	16.3	11.9	1.37	.006
Gastro-intestinal symptoms/ problems				
Nausea/vomiting	17.8	15.4		ns
Unusual color to urine	5.9	5.3		ns
Digestive problems, told by doctor	15.8	15.3		ns
Liver problems, told by doctor	5.9	4.9		ns
Blood symptoms/problems				
Easy bruising/bleeding	10.9	8.7		ns
Swollen glands	10.2	10.3		ns
Anemia, told by doctor	14.7	11.1	1.32	.02
Other blood problems, told by doctor	6.5	4.2	1.55	.03
Skin/Mucous Membranes				
Rash, skin irritation	15.6	13.2		ns
Irritation, burning of eyes and nose	24.0	21.3		ns
Acne	11.0	9.9		ns
Skin disease/allergy, told by doctor	14.5	15.0		ns

TABLE 5-1 (continued)

Outcome N = 1997**	Target % (N = 1049)	Control % (N = 948)	Prevalence Ratio, target/control	p*
Constitutional				
Always fatigued or tired	37.9	32.7	1.16	.02
Bowel complaints (diarrhea/ constipation)	29.5	23.0	1.28	.0013
Loss 10 lbs without dieting	7.0	7.0		ns
Comparative health				
Better	23.6	24.2		
Same	65.6	66.2		ns
Worse	10.9	9.6		
Overall health				
Excellent	21.4	21.9		
Good	51.8	52.5		ns
Fair	21.3	21.1		
Poor	5.2	4.5		
Nervous System				
Dizziness	18.2	16.5		ns
Unconsciousness/blackouts	2.5	2.3		ns
Numbness in fingers, toes	23.6	20.9		ns
Nervousness	30.5	33.7		ns
Headaches	11.3	9.5		ns
Neurologic problems, told by doctor	9.6	11.4		ns
Other				
Kidney problems, told by doctor	11.9	10.0		ns
Cancer, told by doctor	3.6	2.1		ns
Other problems, told by doctor	22.1	16.0	1.38	.0007
Hospitalized since June 1982	11.3	10.1		ns
Seen doctor, last 2 months	33.1	31.1		ns

*Significance at the .05 level, continuity adjusted x^2, 2-tailed.

**Maximum N, which may vary from item to item because of missing values.

TABLE 5-2

Comparison of Target and Control on Crude Prevalence Rates,
Accounting for Within Household Correlation, significant variables

Symptom	p-value*
Wheezing, tightness in chest	.0011
Cough, more than 2 weeks	.0007
Persistent colds	.0025
Irregular Heart Beat	.0335
Heart trouble, told by doctor	.0087
Bowel Complaints	.0029
Anemia, told by doctor	.0229
Other blood problems, told by doctor	.0306
Always fatigued or tired	.0318
Other Problems, told by doctor	.0009

*weighted t-test, 2-tailed.

TABLE 5-3

Association with the target area
multiple logistic regression

	*Coefficient of target/control variable (standard error, s.e.)		p	odds ratio** univariate	adjusted
Wheezing/tightness in chest	-.296	(.113)	.009	1.45	1.34
Cough	-.348	(.115)	.003	1.49	1.42
Persistent colds	-.476	(.162)	.003	1.63	1.61
Irregular heart beat	-.290	(.141)	.04	1.36	1.34
Heart trouble, told by doctor	-.392	(.140)	.005	1.44	1.48
Anemia, told by doctor***	-.299	(.150)	.05	1.38	1.35
Other blood problems, told by doctor***	-.448	(.216)	.04	1.49	1.57
Irritation eyes, nose****	-.263	(.130)	.04		1.30
Always feeling fatigued, tired	-.255	(.100)	.01	1.26	1.29
Bowel complaints (diarrhea/constipation)	-.321	(.108)	.005	1.40	1.38
Other problems, told by doctor	-.419	(.122)	.0006	1.49	1.52

*A negative coefficient for this variable indicates a higher prevalence in the target area as compared to the control.

**Univariate odds ratio based on overall rates presented in Table 5-1. Adjusted odds ratio based on logistic regression, adjusting for possible confounding variables.

***Fail to be significant when quadrant 1 eliminated from control.

****Northeast quadrant of the control area not included in comparison.

TABLE 5-4

Results of Logistic Regressions

Odds Ratio for Significant Variables

Background Variables	Wheezing	Cough	Persist. Colds	Irreg. Heart Beat	Heart Trouble Doctor	Anemia Doctor	Blood Probs Doctor	Eyes Nose	Fatigued, Tired	Bowel Complaints	Other Probs Doctor
Sex (female vs. male)	---	---	1.60	1.47	---	16.89	1.59	---	2.02	2.02	---
Marital Status (Married vs never married)	1.38	---	---	---	---	1.96	---	---	---	---	---
(Wid., Sep, Div. vs never married)	1.46	---	---	---	---	2.48	---	---	---	---	---
Education (not H.S. grad vs college grad)	---	---	---	---	0.44	---	---	---	---	---	---
(H.S. grad vs college grad)	---	---	---	---	---	---	---	---	---	---	---
Work Exposure	1.46	---	---	1.48	1.40	---	---	2.40	1.68	1.55	---
Current Smoker	2.32	2.34	---	1.32	1.39	1.48	---	---	1.42	---	1.38
Former Smoker	1.43	1.45	---	---	1.68	---	---	---	---	---	---
Age (50 vs 20)	1.25	---	---	1.62	2.67	---	1.75	---	---	---	---
Type of House (single family vs other)	---	---	---	---	0.72	---	---	---	0.79	---	---
Target area vs Control area	1.34	1.42	1.61	1.34	1.48	1.35	1.57	1.30	1.29	1.38	1.52

TABLE 5-5

Percentage of respondents with complaints by distance from site

Outcome	Control	Far	Middle	Close	P*	Odds Ratio**
Wheezing	21%	23%	27%	37%	.003	2.06
Chest pain	17	19	18	29	.006	1.79
Shortness of breath	21	21	25	32	.02	1.68
Fatigued, tired	33	37	36	51	.003	1.88
Bruising, bleeding	9	10	11	14	.04	1.95
Headaches	10	8	12	17	.04	1.81
N***	919	355	555	127		

*p-value associated with the distance variable in multiple logistic regressions.

**Odds ratio comparing risk of those close to the site to that of the controls.

***Total N slightly lower than given in Table 4-1 due to missing values.

TABLE 5-6

Interactions between direction and residence in target or control

Outcome	Odds Ratio (Target/Control)			
	Q1	Q2	Q3	Q4
Irritation, eyes, nose	0.40*	0.71	0.56	0.99
Digestive problems	0.78	1.54	0.39*	1.17
Neurologic problems	0.69	0.38	0.22*	0.69

*Significantly different than other quadrants, $.01 < p < .05$.

SECTION 6

FINDINGS: CHILD HEALTH

Tables 6-1 and 6-2 give comparisons between target and control areas for acute and chronic conditions in children of the respondents' households. The information was supplied by the most knowledgeable adult, usually the mother. Questions concerned childhood illnesses and conditions and were not the same as the adult questions. Analyses accounting for within household correlation is presented in Table 6-3. Differences between target and control areas with respect to sore throats are significant. Differences between areas with respect to earaches are not significant after accounting for within-household correlation.

For chronic conditions, only those whose onset was 1974 or after were compared (1974 was the year operations at the Silresim Site became a neighborhood nuisance). Bronchitis more than three times per year, a medical history of a problem that was not one of the usual childhood illnesses, and a loose collection of developmental/ behavioral conditions were all associated with the site. Analysis accounting for within-household correlaton (clustering in households) is given in Table 6-4. Developmental problems lose significance but bronchitis and a history of medical problems still remain at significantly higher prevalence in the target area.

Control of Confounders

Multiple logistic regression was used to control for a variety of background variables as in the adult analysis. The control variables were marital status of the mother (currently married or not), mother's education, mother's workplace exposure, a current smoker in the household (yes/no), type of house, number of persons in the family, maternal age at child's birth, sex of child, ethnicity, drinking habits of mother, and, for the acute conditions of pre-school children, whether they attended group daycare or not. Tables 6-5 and 6-6 reveal that control of these confounders caused the developmental difficulties to lose significance but did not eliminate the elevated prevalence of sore throats, bronchitis, or non-ordinary childhood diseases of various organ systems. Again, the types of non-usual childhood diseases were too rare for statistical analysis. There was no difference between areas in earaches or frequent colds among pre-school children. Epilepsy and asthma were also too rare for the multiple logistic regression to be meaningful. There was no difference in school absenteeism in the two groups.

There were no significant associations with distance or direction for these conditions, nor were there significant interactions among the site-associated variables.

The findings in the child health section of the study, then, suggest a higher site-associated prevalence of sore throats for pre-school and school aged children, and an increased prevalence of at least one year of bronchitis three or more times since 1974. The adjusted odds ratios comparing target to control areas are 3.04, 1.79 and 2.81, respectively. There was no apparent distance or direction effect. Target area children also had a history of the less-usual childhood illnesses more often (odds ratio 2.27), but the data do not allow a statement whether this is due to a particular ailment or group of ailments. The question of bias will be treated in Section 8.

TABLE 6-1

Crude Comparisons for Acute Conditions, Pre-School and School Age Children

Outcome	Target %	Control %	95% confident interval for prevalence ratio	p*
Children, 0 - 5	(N = 181)**	(N = 170)		
Earaches, > 2x/2 mos.	11.7	7.7	.79 - 2.93	ns
Sore throats, > 2x/2 mos.	13.8	4.7	1.36 - 6.33	.001
Frequent colds, past year	27.6	27.6	0.71 - 1.40	ns
Children, 6-17	(N = 395)	(N = 313)		
Earaches, > 2x/2 mos.	7.6	4.5	0.91 - 3.13	.06
Sore throats, > 2x/2 mos.	24.1	14.1	1.24 - 2.36	.001
Frequent Colds	24.6	19.2	0.96 - 1.70	ns

*x^2-test, 2-tailed, ns > .05

**Sample sizes somewhat reduced by missing values.

TABLE 6-2

Crude comparisons, chronic conditions diagnosed 1974 and after

Outcome	Target %	Control %	p*
Epilepsy	.9	.8	ns
Asthma	4.7	4.3	ns
Bronchitis, > 3x in single year	9.9	3.8	.0003
Developmental			.01
Learning disability	4.6	3.6	
Hyperactivity	4.4	2.6	
Mental retardation	.3	.2	.01
Hearing loss/speech impediment	4.2	1.2	
"Slow"	1.2	1.4	
Diagnosed as having bone, blood, lung, skin, glandular, or other problems that are not one of common childhood diseases	10.9	5.8	.006
Birth defect	4.7	3.5	ns
	(N = 576)	(N = 483)	

*x^2-test, 2-tailed, ns > .05

TABLE 6-3

Crude Comparison for acute conditions,
accounting for within household correlation

Symptom	p-value*
Children, 0-5	
Sore throats	.0030
Children, 6-17	
Earaches	.1499
Sore throats	.0094

* weighted t-test, 2-tailed

TABLE 6-4

Crude comparison of chronic conditions,
accounting for within household correlation

Symptom	p-value*
Bronchitis	.0009
Developmental problems	.0680
Diagnosed as having other problems	.0062

*weighted t-test, 2-tailed

TABLE 6-5

Multiple logistic regression, child health associated with the site acute conditions

Outcome	Coefficient of site variables	(s.e.)	p*	odds ratio
Children, 0-5				
Earaches	-.326	(.386)	ns	----
Sore throat	-1.115	(.444)	.01	3.04
Frequent colds	.041	(.254)	ns	----
Children, 6-17				
Earaches	- .710	(.368)	.054	2.03
Sore throat	- .582	(.218)	.008	1.79
Frequent colds	- .161	(.206)	ns	----

*ns > .05

TABLE 6-6

Multiple logistic regression, child health
Association with the site, chronic conditions
with onset 1974 and after
children, aged 0 - 17

Outcome	Coefficients of target-control variables (s.e)		p-value	odds ratio
Bronchitis, > 3x in a year	-1.033	(.301)	.0006	2.81
Developmental problems	- .382	(.217)	.08	1.47
Diagnosed medical problem	- .819	(.260)	.002	2.27

SECTION 7

REPRODUCTIVE OUTCOMES

In this section, we discuss the findings for health outcomes related to reproductive history in women.

Several outcomes with possible relationship to toxic waste site exposure were examined. These included a history of ever being pregnant; number of live births; history of spontaneous abortion ("miscarriage"); history of stillbirth; history of low birthweight child; and history of menopause. Events occurring only after 1974, the first year of effective exposure, were considered. Multiple logistic regression was performed to adjust for the effect of several potential confounders including education, smoking during pregnancy, maternal age, drinking, ethnicity, type of housing, number of persons living in household, sex of child (for live births).

There was no univariate association between residence in the target area and any of the following variables: ever pregnant, number of live births, spontaneous abortion, or stillbirth (Table 7-1). Multivariate analysis did not alter the lack of significant association between exposure and any of these variables (Table 7-2).

With respect to birth outcome for live infants, variables pertaining to birth defects and birthweight were examined. Neither univariate nor multivariate analysis yielded a significant association between residence in the target area and history of birth defects (Tables 7-1 and 7-2). For birthweight, whether analyzed as a continuous variable or as a dichotomous variable (birthweight less than 2500 grams), there was no significant difference between the target and control groups in univariate analysis (Table 7-1). However, multivariate analysis, indicated that the target group tended to have heavier babies (Table 7-2) after accounting for confounding variables. There is no evidence from these data that maternal exposure is associated with <u>lower</u> birthweight. History of menopause was independently and negatively correlated with exposure: that is, the control population had a greater proportion of women with a history of menopause, even after age of menopause was accounted for (Tables 7-1 and 7-2).

In summary, there was no demonstrated association between adverse reproductive outcome (based on maternal report) and residence near the site. Because of sample size considerations, the actual prevalence ratios could be as high as those listed in Table 7-1 (95% confidence intervals).

TABLE 7-1

Univariate Analyses: Reproductive History by Exposure

Reproductive History Variable	Target %	Control %	95% confidence intervals for prevalence ratios	P-value*
Of All Women	(N = 545)	(N = 493)		
Ever pregnant	77.3	76.3	0.95-1.08	ns
With more than 2 children	40.3	34.9	0.99-1.35	ns
Post Menopause	25.0	30.1	0.68-1.01	0.093
Of women who have been pregnant	(N = 421)	(N = 376)		
Miscarriage	27.9	28.0	0.80-1.25	ns
Stillbirth	4.5	6.1	0.41-1.33	ns
Of live deliveries since '74	(N = 311)	(N = 264)		
Birth defects	4.7	3.5	0.60-3.01	ns
Low birthweight (< 2500g)	9.4	7.3	0.74-2.23	ns**

*Chi-square, two-tailed test.

**For continuous birthweight, target mean = 3338g, control mean = 3279g, ns.

TABLE 7-2

Comparison of Target and Control Areas: Multiple Logistic Regression

Reproductive History Variable	Coefficient for Exposure**		p - value
Ever pregnant	0.086	(.208)	ns
Number of children	.004	(.130)	ns
Miscarriage	0.095	(.170)	ns
Stillbirth	0.324	(.352)	ns
Birth defects	0.529	(.460)	ns
Low birthweight (dichotomous)	-0.226	(.334)	ns
Birthweight (continuous)	-90.3	(50.2)	0.073
Menopause	-0.246		ns

*Ordinary least squares was used to analyze the continuous dependent variables, number of children and birthweight. Low birthweight (dichotomous) and the other dichotomous variables were analyzed with multiple logistic regression.

SECTION 8

DISCUSSION OF SYMPTOM PREVALENCE STUDY

The study findings have shown that there are higher prevalences of some symptoms and not others in the target area compared to the control area. These differences could not be explained by differences in the demographic make-up, occupational experiences or lifestyles of the respective target and control populations. To put these findings in the proper perspective, they must be viewed in the context of the limitations of this study, and before it can be concluded that these differences are "real," possible sources of bias must be considered.

Only residents of the target area still living there at the time of the field study (February - April, 1983) were included as part of the survey. The worst conditions, by resident testimony, occurred in the years 1974 to 1979, some years prior to the study. Conditions are also alleged to be worse during the summer months while this survey took place in winter and early spring. Moreover, exposures among residents varied widely, depending upon the amount of time spent in the neighborhood in an average day, movement within the neighborhood, and relative amount of time spent indoors versus outdoors. Nor was there information on possible sources of chemical exposure in all parts of the "control" areas. Significant exposures in this comparison group would tend to mask differences caused by site exposure. All of these factors would tend to lessen the study's ability to detect site-associated illnesses.

In addition, there were problems connected with the community's attempts to obtain remedial action at the site and the potential effects of media attention on the survey itself. Approximately three weeks after initiation of the field study the alarming results from the second air-monitoring study suddenly brought the Silresim problem to public attention again. This could possibly have increased sensitivities of residents near the site thus accentuating reporting of positive symptoms. This problem is accentuated by the fact that the health outcomes studied are self-reported and most are not verifiable.

It is also possible that there was deliberately false information given by some individuals who wished to influence the results in one direction or the other. While this is always possible, it is unlikely to have had an effect on a population basis, such as was evident here, unless it was part of a very elaborate and widespread community conspiracy. Moreover, those health effects which were of most community concern, namely adverse reproductive effects and increased prevalence of cancer, were not the outcomes revealed by this study to be elevated in the target area.

In general, the study design had sufficient statistical power to detect even modest risk ratios between the two groups. In some instances, where the conditions were rare, as in heart disease among children or causes of death in the two areas, the numbers were too small to make meaningful statements. In addition, the conditions of the study generally could not support a confident analysis concerning the influence of direction and duration in relation to the site. While a number of significant differences in prevalence were revealed, it must be remembered that many comparisons were made and that some differences due purely to random fluctuations might be expected. To offset this, a more conservative two-tailed statistical test was used in place of a one-tailed test when reporting p-values.

The higher prevalences of symptoms of respiratory irritation and headaches are consistent with the known effects of the volatile chemicals at the site (see Appendix 1). In occupational exposures these symptoms only appear after exposures that are much higher than those measured by the air-monitoring

studies, often several thousand times higher. While the most thorough studies were made just after heavy rains had inundated the site, and this could have reduced air emissions to some extent, it is doubtful that the levels were ever high enough to account for the observed effects as reported in the occupational health literature. The third and most comprehensive of these studies was conducted during the field survey itself and is entirely consistent with the first DEQE study which was carried out during dry and hot August weather in 1982. Hence it is unlikely that the air-monitoring studies have grossly misrepresented the exposure conditions. This implies either that the observed differences are not real but rather the result of some systematic bias, or that effects occur in community populations at levels dramatically lower than in occupational groups, or some combination of these two.

With regard to bias, there are several possible sources. In any interview administered survey, bias introduced by the interviewer must be considered. The quality control measures used by the survey research staff at the Center for Survey Research, the sub-contractor that carried out the data collection procedures, have been detailed in Section 3. There is no compelling reason to suggest that there was significant interviewer bias.

While the response rates for this study (79% and 83%) are as good or better than comparable studies elsewhere, this does not answer the question as to whether they are adequate to prevent selection bias. Examination of the reasons for non-response in the two groups (see Section 3) does not indicate any differences, nor was the degree of cooperation, as perceived by the interviewer, different in the two populations. There is thus no good evidence that selection bias is a factor.

The introduction of spurious associations by differentially distributed background (confounding) variables has already been discussed. Multiple logistic regression was used to control for the usual confounders of age, race, sex, income/education, and smoking, as well as some less usual ones such as occupatioanl exposure, but it is still possible that an as yet unidentified confounder exists which is associated both with residence in the target area and health outcome. While this is possible, it is unlikely to have produced the levels of effects seen in this study. However, there is no way to rule out the existence of unknown confounders.

It is possible that there has been differential out-migration in the two areas. In fact, there is substantial anecdotal evidence of this. Many residents of the Silresim area have told of neighbors who had become ill and decided to move away from the vicinity of the site. Site-associated mortality would have the same effect, which would be to <u>decrease</u> the strength of association on a cross-sectional survey. Thus, this would not explain away the observed differences.

The most serious bias problem in the study appears to be the possibility that there is either an exaggerated importance given to symptoms in the target area, or "differential forgetting" in the control area ("recall bias") either of which could lead to the production of apparent differences when there are, in fact, none. Some conditions are more subject to this than others. This kind of bias cannot be statistically "analyzed out" of data. One can, however, examine the data for evidence of bias, and use other information to help decide if the observed effect is the result of bias or biological factors.

Recall bias is caused by the fact that individuals for whom a symptom has a special meaning, or salience, will remember it better than those who do not invest it with this importance. For example, if someone believed their headaches were caused by breathing chemicals in the air of their neighborhood they will remember headaches better than someone for whom a headache is just another minor event in life. The real difficulty lies in the fact that recall bias and biological effect may be highly correlated, i.e., that people who believe their headaches are caused by chemicals in the air may very well be correct. One would expect recall bias to increase with proximity to the site just as one would expect a biological effect to increase. Given the subjective nature of many of the symptoms, we cannot rule it out completely. The best that can be done is to make a judgement on its importance in this instance.

Some evidence that recall bias is operating can be seen in Table 4-1 which gives comparisons of prevalences between target and control areas. Even when the differences do not reach significance one sees an overwhelming trend of higher prevalence in the target group. It is most unlikely that any pollutant or set of pollutants would have such a "pan-symptom" effect. On the other hand, the univariate comparison is confounded by more smoking in the target area and a slightly older population which might account for this.

Another persuasive line of evidence in favor of recall bias can be seen in cross-tabulations which compare respondents who answered "yes" to questions about whether they believed the air or water in their neighborhoods made them ill. Such a belief would be the foundation for a site-associated recall bias if there were more such people in the target area as compared to the control. Table 8-1 gives some results for a few outcomes. Other outcomes are similar. This table shows that there is a much higher symptom prevalence in those people who believed the air or water in their neighborhood made them ill. This was true in <u>both</u> the target and control areas. While this could be interpreted as strong evidence of recall bias, a true biological effect could also have brought respondents to the conclusion that the air or water was responsible for their feelings of ill health.

A better test would be to assess the effect of the site variable on those individuals who do <u>not</u> believe the air or water makes them ill. These individuals are much less likely to be subject to the kind of recall bias that would cause a false association with the site because it is the essence of such bias that respondents associate their symptoms with something that is also associated with the site, which in this case would be bad air or water. Accordingly multiple logistic regressions were done using only those people who answered "no" to <u>both</u> questions about air and water. Both "bowel complaints" and "cough" survived this test, but none of the other outcomes reached significance under these conditions. It should be emphasized that <u>passing</u> the test is some evidence <u>against</u> recall bias, but that failing the test merely means that we are not helped in deciding between the alternatives of biological effect and recall bias. Another use of these responses would be to check for a biological gradient, as in Table 5-5, but this time using only those respondents who answered "No" to the question concerning neighborhood air and illness. Table 8-3 gives these results. Three of the six symptoms remain statistically significant, and of the three others that fail to be significant, all show a biological gradient. Loss of significance would be expected because of the reduced sample size. Despite this, the results are strong evidence that there are factors operating independent of recall bias.

A second test possible from the data in this study derives from the fact that during the interview some respondents spontaneously mentioned the site or

the main community group that organized around the issue of the site, Mass Fair Share. If we denominate such respondents as "aware", we can ask if controlling for "awareness" affects the significance of residence in the target area for our outcome variables. Table 8-2 gives the outcomes and the significance levels for those items which remained significant after controlling for awareness. This test could be viewed as one that could also shed some light on the bias introduced by exaggerated reporting, although one might question whether the "awareness" variable is an adequate proxy for this. On the other hand, the community group also engendered some negative reactions in the neighborhood, so this kind of bias could go in either direction. In any event, failure to pass this test could also be the result of "reverse causation", i.e., actual site-caused sickness induced awareness of the site and concern with it.

Thus, eight of eleven outcomes pass at least one of these tests for lack of recall bias. They are: headaches, wheezing/tightness in the chest, bowel complaints, cough, fatigue, persistent colds, heart problems, and "other" medical disorders. Three outcomes, irregular heart beat, medical history of anemia and history of other blood problems fail both tests and may be considered suspect. In the child health data "sore throat" in pre-school and school age children and "bronchitis" remain significant after controlling for parent's awareness.

The outcomes significantly associated with residence in the target area thus show both evidence of recall bias and real effects. While bias cannot be excluded, the evidence is consistent with a site-associated factor independent of recall bias. The adverse health effects of most community concern, cancer and adverse reproductive outcomes, did not show any increased prevalence. Lastly, the effects of other background variables such as smoking (see Table 5-4) are consistent with existing epidemiologic knowledge and give added confidence in the appropriateness of the analytical procedures. Table 8-4 summarizes some of the factors that might tend to bias the results in one direction or another.

TABLE 8-1

Outcome	Control Area: Opinion as to whether water causes illness Yes % (N = 71)	Control Area: Opinion as to whether water causes illness No % (N = 835)	Target Area: Opinion as to whether water causes illness Yes % (N = 89)	Target Area: Opinion as to whether water causes illness No % (N = 921)
Wheezing	43.7	18.0	49.4	24.6
Chest pains	39.3	16.9	40.9	15.1
Nausea/vomiting	32.4	13.8	37.1	15.9

Outcome	Control Area: Opinion as to whether air causes illness Yes % (N = 33)	Control Area: Opinion as to whether air causes illness No % (N = 875)	Target Area: Opinion as to whether air causes illness Yes % (N = 126)	Target Area: Opinion as to whether air causes illness No % (N = 887)
Wheezing	66.7	18.3	56.3	23.2
Chest pains	48.5	15.8	35.7	16.8
Nausea/vomiting	24.2	14.9	29.4	16.1

TABLE 8-2

Site association after controlling for confounders and "Awareness" of site by means of multiple logistic regression

Outcome	p*
Wheezing	.04
Bowel complaints	.005
Cough	.01
Fatigue, tiredness	.04
Persistent colds	.005
Heart problems	.009
Other medical disorders	.001
Irregular heart beat	ns**
Anemia	ns
Other blood problems	ns

*Multiple logistic regression
**p > .05

TABLE 8-3

Percentage of respondents who did not believe the air made them ill by distance from the site

Outcome	Control	Far	Middle	Close	P-Value*
Wheezing, tight chest	19.0%	19.4%	24.5%	30.4%	.009
Chest pains	16.6	17.1	15.4	25.5	.10
Shortness of breath	19.7	18.9	22.3	24.5	.40
Fatigued, tired	31.3	32.4	33.3	47.1	.02
Bruising, bleeding	8.3	9.3	9.6	12.8	.46
Headaches	9.0	5.9	9.8	14.7	.04
N**	923	322	499	102	

*x^2 2-tailed

**Total N's vary because of missing values.

TABLE 8-4

Sources of bias	Bias in favor of association with site	Bias against association with site
Only current residents part of study		x
Dilution of more exposed with less exposed in target group		x
Assumption that control area is free of comparable chemical exposure		x
Differential out migration from target area		x
Alarming publicity about site during field survey	x	
Recall bias	x	

SECTION 9

MORTALITY STUDY

A review of mortality, as revealed in death certificates, was also conducted. This was meant to gather some crude evidence about possible events in the past that might have affected neighborhood health but which were no longer measureable.

Using a computer generated list provided by the Division of Health Statistics and Research of the Massachusetts Department of Public Health, the death certificates of all Lowell residents who died between January 1, 1970 and December 31, 1981, were examined to see if the decedent's address at the time of death fell into either the designated target or control areas. There were 13,184 deaths in Lowell during this period. Copies of each death certificate were made when a target or control address was encountered. A list of out-of-state deaths was also obtained and appropriate vital registrars contacted by mail to see if any of these out-of-state deaths were individuals from the target or control neighborhoods. Because of a variety of confidentiality restrictions operating in some states, not all such certificates could be obtained. However, the number of these deaths that will fall into one of the two study areas is probably very small and is unlikely to affect the final results. Table 9-1 shows the data ascertainment as of August 31, 1983.

Table 9-2 gives the results for various causes of death in the target and control areas. Effects on mortality would have to be extremely powerful to be detected in this way. Therefore, it was not surprising that there were no statistically significant differences in proportional mortality between the two areas. Because of small numbers, actual rates and trends over time could not be obtained.

TABLE 9-1

Mortality Study

Total Lowell Resident Deaths January 1, 1970 - December 31, 1981	13,184
Out-of-state deaths, 1970-81	266
Out-of-state certificates obtained by August, 1983	66
Number of these deaths in target	0
Number of these deaths in control	3
Total number of deaths in target	191
Total number of deaths in control	300
Total number of deaths, target and control	491

TABLE 9-2

Comparison of Target and Control Areas on Cause of Death

	Target	(percent)	Control	(percent)
All cancers	28	(21.7%)	35	(19.3%)
(leukemia)	1	(0.8)	0	----
Diabetes	0	---	9	(5.0)
Hypertension	0	---	3	(1.7)
Ischemic Heart Disease	52	(40.3)	70	(38.7)
Cerebrovascular	7	(5.4)	7	(3.9)
Artery	5	(3.9)	3	(1.7)
Pneumonia	6	(4.7)	5	(2.8)
Bronchitis	2	(1.6)	1	(0.5)
Cirrhosis	1	(0.8)	4	(2.2)
Motor accidents	3	(2.3)	6	(3.3)
Other accidents	2	(1.6)	3	(1.7)
Suicides	0	---	0	---
Other	23	(17.8)	35	(19.3)
Total*	129		181	

*deaths between 1974 - 1981

Fisher's exact tests show no significant difference in proportion of death due to any specific cause. The proportional mortality rates were not adjusted for age.

SECTION 10

CONCLUSIONS

This study tested the hypothesis that adverse health outcomes occurred with higher prevalence in the area of the Silresim Waste Site compared with a similar area where there was no exposure to hazardous chemicals. We found a statistically significant increase in prevalence for a number of self-reported respiratory and constitutional symptoms in adults and for sore throats and bronchitis in children. There was a strong suggestion that proximity to the site was positively associated with self-reporting of a number of adverse health outcomes in adults. There appeared to be an independent source of irritants associated with some industrial facilities in the control area as well. We found no significant increase in reproductive outcomes or mortality, although the sample sizes were in general small which would make demonstration of differences difficult.

While the effects are modest in size (Tables 5-4 and 6-6), the conditions are of relatively high prevalance and hence could have considerable public health impact in terms of numbers of people affected. Moreover, the effects of other significant health variables such as smoking are roughly the same magnitude as result from distance in the target area.

It is possible that the elevated frequencies of respiratory and constitutional symptoms do not result from contamination of the site but from some undetected bias or some yet unidentified confounding factor. If it is accepted that bias does not fully explain the differences we can ask whether the findings can be interpreted as evidence that exposure to chemicals emanating from the site caused the increased prevalences, using the classical criteria of Hill:

Consistency: The reported symptoms are not dissimilar to those reported anectodtally in other comparable circumstances. On the other hand, more formal studies have often failed to reveal statistically significant differences, although most can be faulted on methodological grounds, as can the present study. The results presented here, then, can not be said to be part of a consistent body of evidence on the effects of relatively low levels of airborne chemicals such as those present at Silresim.

Temporal Sequence: Since this was a cross-sectional study involving simultaneous measurement of exposure (given by current residence near the site) and outcome (given by self-reported symptoms) we can not determine temporal order. In many instances the reported symptoms could have predated exposure.

Specificity: The symptoms reported to be in excess in the Silresim neighborhood are not specific for the chemicals present at the site, or even for chemicals in general, since they can be caused by many other agents or factors.

Coherence with other evidence: There are not studies that suggest that exposure to chemicals at levels as low as found in the Silresim neighborhood have any health effects. Occupational studies indicate that the symptoms reported by neighborhood residents would only appear at concentrations much higher than those revealed by the air-monitoring studies. Working populations are quite different than community populations, and are less often studied for the less severe health effects present here. Moreover, the occupational exposures are usually confined to one or a few agents, not the heterogeneous mixture present at Silresim. Experimental evidence does exist for synergistic interactions, but this remains speculative in the present case. Thus, there is only very weak coherence with other evidence.

Strength of Association: The magnitude of the odds ratios ranges from weak (up to 1.50) to moderate (1.5-3.0). When those closest to the site are compared to controls, the association is of moderate strength (see Table 5-5). It is unlikely that an unknown confounder could account for this.

Biological Gradient: Wheezing and tightness in the chest, chest pains, shortness of breath, unusual fatigue and tiredness and headaches all increased in prevalence with closeness to the site. The fact that this was true also for those residents who did not believe the air in this neighborhood was unhealthy is an argument against recall bias (Tables 5-5 and 8-3). The biological gradient is strongly suggestive of an exposure related effect.

Statistical Significance: The use of tests of statistical significance merely insures that the observed differences are unlikely to be a result of random fluctuation induced by sampling, and has no real bearing on the question of causality.

In summary, investigation of the Silresim area neighborhood did not reveal any unusual patterns of mortality, increased cancer risk, or birth defects or other reproductive hazards. Much less severe but still troublesome health effects did seem to be more common in the vicinity of the site. However, the observed effects seem to be out of proportion to the apparent exposure. Thus it is not possible to state conclusively that the site is the cause of the adverse health effects.

APPENDIX 1

SUMMARY OF HEALTH EFFECTS OF SOME VOLATILE CHEMICALS PRESENT AT THE SILRESIM SITE USED TO CONSTRUCT SURVEY INSTRUMENT

(see Section 2)

Three serious pathological conditions have been associated with inhalation of benzene and all three may be of concern low level exposure. They are leukemia, especially acute myelogenous leukemia; pancytopenia, including aplastic anemia; and chromosomal breakage and aberration. Blood and blood forming organs, therefore, appear to be a critical target. Benzene binds covalently to solid residues in bone marrow and its metabolites accumulate there. Most of our knowledge about benzene toxicity is derived from occupational exposures and little is known about the effects of low levels. Chromosomal breakage and rearrangements have been consistently noted at levels between 25 ppm and 150 ppm, but significant effects in at least one study were seen at 2-3 ppm for an 8 hour time-weighted average (TWA) (corresponding to less than 1 ppm on a 24 hour basis). The effects persist in lymphoid and hematopoietic tissue after exposure ceases. Other effects at relatively low levels include increased red blood cell levels of the heme precursor, delta aminolevulinic acid, and a decrease in mean serum complement levels. Both of these effects occur at between 3 and 15 ppm. Their clinical significance is unknown.

Chlorobenzene exposures produce headache and irritation of the eyes and upper respiratory tract in occupational exposures. Acute toxic effects on the heart have been described and include precordial pain, bradycardia and other rhythm disturbances, and hemodynamic changes. The exposure levels associated with these clinical manifestations are unclear.

Ethylbenzene seems to have few toxic effects until levels are extremely high. Eye irritation does not appear until levels reach 1000 ppm.

Straight-chain chlorinated hydrocarbons present at the site include chlorinated ethylenes, chlorinated ethanes and halomethanes. The chlorinated ethylenes are suspected carcinogens. Rats and guinea pigs exposed to high levels also suffer liver and kidney damage. Continuous 90 day exposures at 5 ppm have caused increased mortality in guinea pigs. Some increase in cardiac irritability has been demonstrated at high levels.

Tetrachloroethylene and trichloroethylene are both widely distributed in the environment, although generally at low levels. Their lipid solubility and high octanol/water partition coefficients suggest a tendency to bioaccumulate. As with all members of the chloroethylene family, acute effects are dominated by depression of the central nervous system. This occurs only at high levels, however. There is also some evidence that cardiac sensitization can take place. Both of these compounds cause liver cancers in animals, and significant cancer risks are conceivable at less than 1 ppm. This is based, however, on low dose extrapolation in a multi-stage risk estimate model, and the appropriateness of this procedure has been questioned.

Four chlorinated ethanes are present at the site, the 1,2,1,1-, 1,1,1,2- and 1,1,2,2,- species. The most toxic is 1,2-dichloroethane, followed by 1,1,2,2-tetrachloroethane, 1,1,2-trichloroethane and 1,1,1-trichloroethane. The first three are considered by NIOSH to be possible human carcinogens. Again, toxic effects seem to appear mostly at high levels, and they include gastrointestinal symptoms and CNS problems such as headache, tremors and dizziness.

Chloroform is a volatile, lipid soluble halomethane that tends to build up in nervous tissue. It is judged to be moderately toxic. NIOSH has recommended (1976) an 8 hour TWA of 2 ppm (i.e., .66 ppm over 24 hours). A single study suggests that working with chloroform might increase the risk of viral hepatitis. Exposures of the workers of this study varied between 2 and 200 ppm.

Various other agents, including alcohol and phenobarbital, increase the toxic effects of chloroform by lowering the threshold for necrotic action on the liver. Chloroform is also an animal carcinogen.

Dichloromethane (methylene chloride) is an unusual compound in that it is metabolized in the body to carbon monoxide. Individuals exposed to this chemical in commercial paintstrippers have suffered myocardial infarctions. The increase in carboxy-hemoglobin also causes severe throbbing headache, and nausea.

APPENDIX 2

QUESTIONNAIRE

Copies of the questionnaire may be obtained by contacting:

Dr. John Cutler, Director
Massachusetts Department of Public Health
Division of Environmental Health Assessment
150 Tremont Street, 7th Floor
Boston, Massachusetts 02111
(617) 727-2660

APPENDIX 3

LETTERS AND FACT SHEETS

Center for Survey Research
100 Arlington Street, Boston, Massachusetts 02116 · (617) 956-1150

Dear Resident of Lowell:

We need the benefit of your experience and hope you will help us. We are carrying out an important study sponsored by the Commonwealth of Massachusetts.

Your household is one of about 1200 in Lowell selected as part of a scientific sample in the City of Lowell for a study of health and the experiences which may affect health.

An interviewer will be calling or visiting your home within the next few weeks to ask to speak with the adults in your household.

There is an information sheet enclosed which may answer questions you have; your interviewer will be glad to answer additional questions.

Let me just assure you of three points. First, although your help is, of course, voluntary, the sample method makes the cooperation of each selected household very important to producing accurate data. Second, your answers are strictly confidential. Third, by giving your time you will be contributing to a project that will be very valuable to the community of Lowell and for people in other cities.

Sincerely,

David Ozonoff, MD

David M. Ozonoff, M.D.

Mary Ellen Colten

Mary Ellen Colten, Ph.D.

INFORMATION SHEET
LOWELL HEALTH STUDY

Who is doing the study? The study is being conducted by the Center for Survey Research of the University of Massachusetts and the School of Public Health of Boston University. The Commonwealth of Massachusetts is sponsoring the study.

What is this study for? We are trying to learn more about the health of people in Lowell and about things in their lives which may affect their health -- such as where they have lived and what kind of work they have done.

How were households selected? A sample of over 1200 households in various sections of Lowell were selected. Within each section each household had an equal chance of being chosen. The scientific method used means that information from each household selected is important to the accuracy of the research. Once a selection is made we can make no substitutions. It is important to be sure that the people interviewed as a group represent the residents of the area.

Who in the household will be interviewed? The interviewer will ask to speak separately with each adult in your household. The interview will take about 20 minutes of your time.

Who will see my answers? Your answers are completely confidential. Your name will not be attached to your answers. The only people who will see your answers to the questions will be the scientists working on the study and their assistants. When we present what we found from the study, we report what groups of people said as a whole. When the results are reported, everyone's answers will be put together so that no information is given that would allow someone to be identified. We will report things like, "46% of the residents of Lowell told us that ..."

What kinds of questions will be asked? The questions are about your health, your current health, some past health problems, health services you have used, and the things you do that may affect your health, such as what kind of work you do, what kinds of hobbies you have, and where you have lived. They are questions you can answer easily, but the answers taken together provide data that cannot be obtained in any other way. Most people find the questions interesting. However, you may ask the interviewer to skip a question if there is any one you do not want to answer.

What if I have questions? Your interviewer will be happy to answer your questions, or you can call Margaret Raisty in our Field Office at 956-1150.

ADDITIONAL INFORMATION:

Why was Lowell chosen? Because the state is concerned about health problems that are typical of industrial cities like Lowell.

Benefits of the Study. The information will be used by the Commonwealth of Massachusetts in deciding what to do about the problems of people in cities like Lowell.

Where will the data be available? The Massachusetts Department of Public Health and the Department of Environmental Quality Engineering will have a report available by the end of this year.

Center for Survey Research
100 Arlington Street, Boston, Massachusetts 02116 · (617) 950 1150

Caríssimo Residente de Lowell:

Necessitamos que nos ajude com a sua experiência e esperamos obtê-la. Estamos levando a efeito um estudo importante patrocinado pelo Governo de Massachusetts.

A vossa casa é uma das 1200 em Lowell seleccionadas como parte duma amostra científica da cidade de Lowell para um estudo sobre saúde e factores que possam de qualquer forma afectar a saúde.

Um empregado nosso vai chamar ou visitar a vossa casa dentro em pouco para fazer algumas perguntas aos adultos que aí moram.

Junto segue uma folha com informação que talvez seja a resposta a algumas perguntas que queira fazer; a pessoa que vai proceder à entrevista terá muito prazer em responder a quisquer perguntas adicionais.

Permita-me que lhe dê a certeza de 3 coisas: Primeiro, embora a vossa ajuda seja certamente voluntária, o método de exemplo ou amostra torna muitíssimo importante a cooperação de cada casa escolhida, no aspecto de apresentar informação exacta. Segundo, as vossas respostas são absolutamente confidenciais. Terceiro, a dádiva do vosso tempo irá contribuir para um projecto que trará muito valor à comunidade de Lowell e às populações de outras cidades.

Sinceramente,

David M. Ozonoff, M.D.

David Ozonoff, M.D.

Mary Ellen Colten, Ph.D.

Mary Ellen Colten

FOLHA DE INFORMAÇÃO
ESTUDO DE SAÚDE EM LOWELL

Quem está procedendo ao estudo? O estudo está sendo levado a efeito pelo Centro de Pesquisa da Universidade de Massachusetts e pela Escola de Saúde Pública da Universidade de Boston. O Governo de Massachusetts concorre com as despesas deste programa.

Para que é este estudo? Estamos tentando saber mais àcerca da saúde da população de Lowell e àcerca de coisas que poderão afectar a sua saúde -- tais como onde tem vivido e que especie de trabalho tem feito.

Como é que as casas foram escolhidas? Uma amostra de mais de 1200 lares em várias secções de Lowell foi seleccionada. Dentro de cada secção, cada lar teve a mesma oportunidade de ser escolhido. O método científico usado significa que a informação de cada casa escolhida é importante para a exactidão da pesquisa. Uma vez que uma selecção é feita, nós não podemos fazer substituições. É importante ter a certeza de que as pessoas entrevistadas como um grupo representam os residentes da área.

Quem vai ser entrevistado em cada lar? A pessoa que vos vai entrevistar pedirá para falar a sós com cada adulto do vosso lar. A entrevista levará aproximadamente 20 minutos.

Quem vai ver as minhas respostas? As vossas respostas são absolutamente confidenciais. O vosso nome não será posto nas vossas respostas. As únicas pessoas que vão ler as respostas serão os cientistas trabalhando neste estudo e os seus assistentes. Quando nós apresentarmos o resultado do nosso estudo, estaremos apresentando o que disseram grupos de pessoas como "um todo". Assim, as respostas de toda a gente serão postas juntas de tal modo que nenhuma informação poderá ser identificada. Ao divulgarmos o resultado deste estudo será, por exemplo, do seguinte modo: "46% dos residentes de Lowell disseram-nos que..."

Que espécie de perguntas serão feitas? As perguntas serão sobre a vossa saúde, a saúde actual, alguns problemas de saúde do passado, Serviços de Saúde que tenham usado e as coisas que tenham feito que possam ter afectado a vossa saúde, tais como o trabalho, os passa-tempos e onde têm vivido. São perguntas a que podereis responder fàcilmente, mas as respostas colocadas juntas dar-nos-ão informação que de outro modo não poderia ser obtida. A maior parte das pessoas acha as perguntas interessantes. No entanto podem pedir para não responder a certas perguntas se entenderem que o não devem fazer.

E se eu quizer também fazer perguntas? Quem vos vai entrevistar terá muito prazer em responder às vossas perguntas, ou então podereis chamar Margaret Raisty, telefone 956 1150, no nosso Field Office.

Center for Survey Research
100 Arlington Street, Boston, Massachusetts 02116 · (617) 956-1150

Dear Resident of Lowell:

Recently, one of our interviewers asked the adult members of your household to participate in the health study of Lowell being conducted by the Boston University School of Public Health and the Center for Survey Research. At that time, one or more of you indicated that you would rather not be interviewed.

Sometimes people feel this way because of certain concerns they have about these kinds of interviews. Because having the benefit of your experiences and opinions is very important, we would like to take this opportunity to give you some additional information about the study.

Many people wonder how their household was selected. Your household was selected as part of a scientific sample of households in Lowell. The study, which is funded by the Commonwealth of Massachusetts, has been designed to answer certain important questions about community health in highly industrialized cities like Lowell. Only by interviewing every adult in each of the selected households can we be sure that our findings actually represent the health experiences of people in Lowell.

Your participation, of course, is entirely voluntary. If, after starting the interview, there are questions you do not care to answer you may ask the interviewer to skip them. However, your participation is very important to our study and to the accuracy of the results.

Let me assure you again that all responses are kept in the strictest confidence. No information will be released that can identify anyone taking part in the study.

We hope that you might reconsider your participation in this study. The interview is not lengthy. Most people have found it to be a worthwhile and enjoyable experience.

In the near future, an interviewer will contact you again to ask for your participation, and if you wish, arrange for a convenient time to talk to you.

Thank you for your time and consideration.

Sincerely yours,

David Ozonoff, M.D.

Mary Ellen Colten

Mary Ellen Colten, Ph.D.